"additional facts not contained in the paper" read by him to the Linnean Society.

Although Prof. Allman does not directly allude to my article of the week before (p. 147), I may assume that the statements which he makes in opposition to my conclusion that Limnocodium (Craspedacustes) belongs to the group of the Trachomedusæ were elicited by the publication of my results.

I intend in the July number of the *Quarterly Journal of Microscopical Science* to show in an illustrated memoir that, contrary to the conclusion of Prof. Allman, the tentacles of Limnocodium do resemble those of the Trachyline Medusæ in their insertion and in the possession of true (though rudimentary) peronia, as I stated in my original note and in my paper read to the Royal Society on June 17. I shall also show that my statement that the so-called lithocysts or marginal bodies have essentially the same structure as those of Trachyline Medusæ (being modified tentacles with an endodermal axis) is warranted by the developmental history of the bodies in question. Consequently I adhere to my original determination of the affinities of the new Medusa as one of the order Trachomedusæ, and cannot agree with Prof. Allman that its affinity with the Leptomedusæ must be regarded as the closer of the two.

Prof. Allman states that he has arranged certain methods of observation with Mr. Sowerby, by which he hopes to determine the developmental history of Limnocodium. It will be of the greatest interest to have this matter fully investigated, and to know what are the methods which Prof. Allman has devised to this end. Mr. Sowerby informs me that at present he has undertaken no experiments of the kind excepting the isolation of specimens in two glass jars in the lily-house, which he carried out at my special request on June 15.

In the meantime I may say that I have fully satisfied myself that Limnocodium develops directly from the egg. When specimens are kept living in a glass jar under constant observation it is found that exceedingly small specimens of the Medusa make their appearance amongst the larger specimens. Mr. Sowerby had already determined this fact a fortnight ago, when I first was introduced by him to the Medusa. I have now, through his kindness, been able to examine several young phases of Limnocodium, the discovery of which is entirely due to him.

The youngest specimen which I have seen at present measured only the one-thirtieth of an inch in diameter, and I have had others under observation very little larger. The smallest was of a sub-spherical form without any aperture to the ectodermal investment. Four minute tentacles were sprouting near one pole of the spherical body, and between these rudiments of four others were seen. Within—the subumbrellar musculature was already developed and contracting at intervals. The four radial canals were also present, and, what is more remarkable, the sub-umbrellar cavity was already well marked, and within it the manubrium with the oral aperture. Yet the margin of the umbrella was still closed by a continuous ectodermal coat which, when perforated, would, I conceive, become the velum.

These minute embryos correspond very closely in appearance with the embryos of the well-known typical Trachomedusan Geryonia, as figured by Metschnikow in the *Zeitsch. für wiss. Zoologie*, vol. xxiv., Plate II., Figs. 12 and 15.

They leave no possibility of supposing that Limnocodium has, like most Leptomedusæ, a hydroid trophosome. In respect of its development as in other respects, Limnocodium is not more closely allied to the Leptomedusæ than to the Trachomedusæ, but is one of the Trachomedusæ.

A remarkable fact which I am not able to explain is the excessive rarity of females amongst the specimens of Limnocodium taken from the tank in Regent's Park. All the specimens which I have examined have been males. Females clearly enough must be present, or have been present amongst the shoals of males—whence the embryos discovered by Mr. Sowerby.

It is a known fact among Trachyline Medusæ that in some species males are excessively abundant, and even in some species females have never been detected. Thus again Limnocodium agrees with the Trachyline Medusæ.

One word more with regard to the name of the new Medusa. Whilst I waive the right of priority for the generic term Craspedacustes, and adopt Prof. Allman's term Limnocodium, I feel it to be only right to maintain the association of Mr. Sowerby's name with this discovery, which I had originally proposed, and I shall accordingly henceforth speak of the Medusa as *Limnocodium Sowerbii*, Allman and Lankester.

E. RAY LANKESTER

Aqueous Vapour in Relation to Perpetual Snow

SOME twelve years ago I gave (*Phil. Mag.*, March, 1867, "Climate and Time," p. 548) what appears to be the true explanation of that apparently paradoxical fact observed by Mr. Glaisher, that the difference of reading between a thermometer exposed to direct sunshine and one shaded *diminishes*, instead of increases, as we ascend in the atmosphere. This led me to an important conclusion in regard to the influence of aqueous vapour on the melting-point of snow; but recent objections to some of my views convince me that I have not given to that conclusion the prominence it deserves. I shall now state in a few words the conclusion to which I refer.

The reason why snow at great elevations does not melt but remains permanent, is owing to the fact that the heat received from the sun is thrown off into stellar space so rapidly by radiation and reflection that the sun fails to raise the temperature of the snow to the melting point; the snow evaporates, but it does not melt. The summits of the Himalayas, for example, must receive more than ten times the amount of heat necessary to melt all the snow that falls on them, notwithstanding which the snow is not melted. And in spite of the strength of the sun and the dryness of the air at those altitudes, evaporation is insufficient to remove the snow. At low elevations, where the snowfall is probably greater and the amount of heat even less than at the summits, the snow melts and disappears. This, I believe, we must attribute to the influence of aqueous vapour. At high elevations the air is dry and allows the heat radiated from the snow to pass into space, but at low elevations a very considerable amount of the heat radiated from the snow is absorbed in passing through the atmosphere. A considerable portion of the heat thus absorbed by the vapour is radiated back on the snow, but the heat thus radiated, being of the same quality as that which the snow itself radiates, is on this account absorbed by the snow. Little or none of it is reflected like that received from the sun. The consequence is that the heat thus absorbed accumulates in the snow till melting takes place. Were the amount of aqueous vapour possessed by the atmosphere sufficiently diminished, perpetual snow would cover our globe down to the sea-shore. It is true that the air is warmer at the lower level than at the higher level, and by contact with the snow must tend to melt it more at the former than at the latter position. But we must remember that the air is warmer mainly in consequence of the influence of aqueous vapour, and that were the quantity of vapour reduced to the amount in question the difference of temperature at the two positions would not be great.

But it may be urged, as a further objection to the foregoing conclusion, that as a matter of fact on great mountain chains the snow-line reaches to a lower level on the side where the air is moist than on the opposite side where it is dry and arid. As, for example, on the southern side of the Himalayas and on the eastern side of the Andes, where the snow-line descends some 2,000 or 3,000 feet below that of the opposite or dry side. But this is owing to the fact that it is on the moist side that by far the greatest amount of snow is precipitated. The moist winds of the south-west monsoon deposit their snow almost wholly on the southern side of the Himalayas, and the south-east trades the snow on the east side of the Andes. Were the conditions in every respect the same on both sides of these mountain ranges, with the exception only that the air on one side was perfectly dry, allowing radiation from the snow to pass without interruption into stellar space, while on the other side the air was moist and full of aqueous vapour absorbing the heat radiated from the snow, the snow-line would in this case undoubtedly descend to a lower level on the dry than on the moist side. No doubt more snow would be evaporated off the dry than off the moist side, but melting would certainly take place at a greater elevation on the moist than on the dry side, and this is what would mainly determine the position of the snow-line.

In like manner the dryness of the air will in a great measure account for the present accumulation of snow and ice on Greenland and on the Antarctic continent. I have shown on former occasions that those regions are completely covered with perpetual snow and ice, not because the quantity of snow falling on them is great, but because the quantity melted is small. And the reason why the snow does not melt is not because the amount of heat received during the year is not equivalent to the work of melting the ice, but, mainly because of the dryness of the air, the snow is prevented from rising to the melting-point.

There is little doubt but that the cold of the glacial epoch would produce an analogous effect on temperate regions to that

experienced at present on Arctic and Antarctic regions. The cold, although it might to some extent diminish the snowfall, would dry the air and prevent the temperature of the snow rising to the melting-point. It would not prevent evaporation taking place over the ocean by the sun's heat, but the reverse, but it would prevent the melting of the snow on the land during the greater part of the year.

In places like Fuego and S. Georgia, where the snow-fall is considerable, perennial snow and ice are produced by diametrically opposite means, as I have elsewhere shown, viz., by the sun's heat being cut off by clouds and dense fogs. In the first place the upper surface of the clouds act as reflectors, throwing back the sun's rays into stellar space; and in the second place, of the heat which the clouds and fogs absorb, more than one-half is not radiated downwards on the snow, but upwards into space. And the comparatively small portion of heat which manages to reach the ground and be available in melting the snow is insufficient to clear off the winter's accumulation.

JAMES CROLL

Artificial Diamonds

ON reading Mr. Hannay's communication to the Royal Society on the production artificially of crystallised carbon or diamond (*Proc.* Roy. Soc., vol. xxx., No. 204, May, 1880), in the course of which Mr. Hannay states that he has made eighty experiments, only three of which have been successful. In almost every case his iron or steel vessels, enormously thick in proportion to their small bore, have burst at a red heat or above it, by the pressure of the included hydrocarbon vapour.

Will Mr. Hannay permit me to suggest to him that if, instead of an enormously thick and difficult to weld up tube, he will inclose his materials in a comparatively thin one and then inclose that in another like tube shrunk on or contracted over the former, and so on to a third, or, if necessary, fourth tube, each possessing an initial tension upon those within it, he may thus obtain compound tubes either of wrought iron or steel easily welded staunch, and capable of withstanding any assignable amount of internal elastic pressure. This is the principle upon which, since 1855, all rifled artillery is constructed.

The Grove, Clapham Road, June 22 R. MALLET

A Fourth State of Matter

IN Mr. Crookes' communication on this subject (NATURE, vol. xxii. p. 153) occurs the sentence, "An isolated molecule is an inconceivable entity." This proposition would appear to me to be questionable. For if we cannot conceive an isolated molecule, how are we to conceive of two (or more) molecules, *i.e.*, conceive of matter at all? For the conception of two molecules involves the isolation of each in the mind, otherwise surely the two would be mentally blended into one. It is further said of a molecule, "Solid it cannot be." May not the external qualities ordinarily attributed to a "solid" be said to be those of a body possessing a certain amount of rigidity (*i.e.*, whose parts resist displacement) combined with a certain elasticity? Would not these be substantially the properties of a single vortex molecule, according to those who have investigated this subject? For it appears that such a molecule would be (perfectly) elastic, and inseparable into parts. At the same time it would seem that there would be nothing to prevent it from being harder or more rigid than any large scale solid (built up of such molecules?) with which we are acquainted.

"A fourth state of matter," as it appears to me, is a distinction which has something arbitrary about it. If (for instance) the æther be a gas, the mean length of path of whose minute molecules is not less than planetary distances—a proposition which it might not be easy to disprove directly—then this would be a mean path indefinitely greater than that of the molecules of the most rarefied gas. Would it, however, be legitimate to regard the æther (under this condition) as matter in "a fourth state"? This would seem, in my judgment at least, only to complicate the subject unnecessarily. For after all we are concerned in such cases with the mere quantitative difference of length of path. S. TOLVER PRESTON

London, June 28

Auroral Observations

IN order to get nearer, if possible, to the unravelling of the mysteries of the aurora borealis, I have in the course of the last two years endeavoured to procure a great number of observations of this phenomenon in Norway, Sweden, and Denmark. I have succeeded in engaging throughout the above-named countries several hundreds of observers, who, led only by scientific interest, have lent me their assistance, and from whom I have already received a considerable amount of information. These observations are to be continued, as there is reason to suppose that the aurora borealis in the near future will appear much more frequently than has been the case during the last years. Finland and Iceland will also now be drawn within the circle, and *it would be most desirable that similar observations were made also in Great Britain*, which country—especially in the maximum years of the appearance of the aurora borealis—certainly would yield characteristic contributions in this respect. I therefore take the liberty to invite friends of science to make such observations in accordance with the system which I have introduced in Scandinavia; a schedule for recording observations, along with the necessary instructions, will be sent to any one who, before the end of August, informs me of his name and address.

SOPHUS TROMHOLT

Bergen, Norway, June Professor of Mathematics

Other papers in Great Britain are requested kindly to give the above appeal a place in their columns.

The Hydrographic Department

As you have been misinformed on several points respecting my connection with the Hydrographic Department, I request, both on public grounds and in ordinary fairness to myself, that you will insert the following corrections of statements in your article on this subject in NATURE, vol. xxii. p. 86.

My work on the Norwegian coast has not been "dignified into a hydrographical survey." That work, combined with my knowledge of the Norwegian language, charts, and pilotage, satisfied the hydrographer that I was competent to compile a "Norway Pilot." It is incorrect to represent that I have ever laid claim to anything more than that.

I have not made a "rude" or "ungenerous" attack on the Hydrographic Department. I have temperately stated facts which cannot be disproved, in the interests of hydrography, and to show the necessity for giving increased strength and efficiency to the department. It is no answer to these facts to disparage my own efforts in the cause, or to call me a small and obscure clique actuated by personal motives. The clique to which I belong is small indeed, for it consists only of myself. It may also be obscure, but it is untrue that I am influenced, in anything I may do, by other than public motives and a desire to further the interests of commerce and of hydrography.

The gravest error into which your informant has led you is the statement that the Hydrographic Department had confided to me, "mistakenly" or otherwise, the "*revision* of the sailing directions" for part of Norway. I *compiled* those sailing directions, as expressly stated in the official printed "Advertisement," signed by the hydrographer himself, and the department has done exactly the opposite of what your informant states; it has refused to allow me to *revise* my own work, and has consequently published an erroneous light list, which will be followed by an incomplete "Pilot." Against this procedure it is my obvious duty to protest. I am also bound to warn all those whom it may concern of the errors to which the department has deliberately given dangerous publicity.

The paper read before the Society of Arts brought the dangers along the trade route between England and Siberia to public notice in some detail, and contained other facts relating to neglected surveys and to charts compiled from antique and inadequate data, which it was right that merchants and seamen should be aware of. If my statements are accurate—and I challenge your informant to disprove any one of them—then the Society of Arts did useful service in accepting my paper. No good end can be gained by calling me names and accusing me of personal motives. Let my statements be disproved if your informant is able to disprove them. If he cannot do so, then those statements are incontrovertible witnesses to the fact that the Hydrographic Department is unequal to the demands upon it. Unsupported assertions that the department stands "well, and deservedly so, in the estimation of scientific circles," are of no weight when opposed to facts, which your informant cannot disprove, and apparently dares not face. GEORGE T. TEMPLE

The Nash, near Worcester, June 2

[WE have given publicity to Lieut. Temple's reply to the